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THE PROTEINS AND COLLOID CHEMISTRY[1]

I

THE proteins, like certain other constituents of protoplasm, are colloidal in character, *i. e.*, they are not able to diffuse through animal membranes which are permeable to crystalloids. For this reason a number of authors have tried to explain the behavior of proteins from the viewpoint of the newer concepts of colloid chemistry. Foremost among these concepts is the idea that the reactions between colloids and other bodies are not determined by the purely chemical forces of primary or secondary valency but follow the rules of "adsorption." Although a number of authors, during the last twenty years, *e. g.*, Bugarszky and Liebermann, Hardy, Pauli, Robertson, Sörensen, and others, have advocated a chemical conception of the reactions of proteins, their experiments failed to convince the other side since these experiments could just as well be explained on the basis of the adsorption theory. There were two reasons for this failure. First, the experiments did not show that ions combined with proteins in the typical ratio in which the same ions combine with crystalloids. This proof only became possible when it was recognized that the hydrogen ion concentration of the protein solution determines the amount of ion entering into combination with a protein, and that therefore the ratios in which different ions combine with proteins must be compared for the same hydrogen ion concentrations. Since the former workers were in the habit of comparing the effects of

[1] Address delivered before the Harvey Society, October 16, 1920. The writer's experiments, on which this address is based, have appeared in the *J. Gen. Physiol.*, 1918–19, I., 39, 237, 363, 483, 559; 1919–20, II., 87; 1920–21, III., 85.

the same quantities of acid or alkali added instead of comparing the behavior of proteins at the same hydrogen ion concentration they were not able to furnish the final proof for the purely chemical character of the combinations between ions and proteins, and nothing prevented chemists from assuming that proteins formed only adsorption compounds with acids, bases, and neutral salts.

The second reason for the failure to prove the purely chemical character of the protein compounds lay in the so-called Hofmeister series of ion effects. Hofmeister was the first to investigate the effects of different salts on the physical properties of proteins, and he and his followers observed that the relative effects of anions on the precipitation, the swelling, and other properties of proteins was very definite and that the anions could be arranged in definite series according to their relative efficiency, the order being independent of the nature of the cation. Similar series were also found for the cations, though these series seemed to be less definite. These Hofmeister series were a puzzle inasmuch as it was impossible to discover in them any relation to the typical combining ratios of the ions, and this lack of chemical character in the Hofmeister series induced chemists to explain these series on the assumption of a selective adsorption of these ions by the colloids.

To illustrate this we will quote the order which, according to Pauli, represents the relative efficiency of different acids on the viscosity of blood albumin,

HCl > monochloracetic > oxalic > dichloracetic > citric > acetic > sulfuric > trichloracetic acid,

where HCl increased the viscosity most and trichloracetic or sulfuric least. In this series the strong monobasic acid HCl is followed by the weak monochloracetic acid, this is followed by the dibasic oxalic acid; later follows a weak tribasic citric acid, then the very weak monobasic acetic acid, then the strong dibasic sulfuric acid, and finally again a monobasic acid, trichloracetic. Pauli is a believer in the chemical theory of the behavior of proteins but it is impossible to harmonize his series of anions with any purely chemical theory of the behavior of proteins.

The ion series of Hofmeister are no more favorable for a chemical conception. Thus, according to Hofmeister, gelatin swells more in chlorides, bromides and nitrates than in water, while in acetates, tartrates, citrates, or sugar it swells less than in water. R. Lillie arranges ions according to their depressing effect on the osmotic pressure of gelatin solution in the following way,

$$Cl > SO_4 > NO_3 > Br > I > CNS.$$

These series again betray no relation to the stoichiometrical properties of the ions. As long as these Hofmeister ion series were believed to have a real existence it seemed futile to decide for or against a purely chemical theory of the behavior of colloids since even with a bias in favor of a chemical theory the Hofmeister series remained a puzzle.

The writer believes to have removed these difficulties by using protein solutions of the same hydrogen ion concentration as the standard of comparison. In this way he was able to show that acids, alkalies, and neutral salts combine with proteins by the same chemical forces of primary valency by which they combine with crystalloids, and that, moreover, the influence of the different ions upon the physical properties of proteins can be predicted from the general combining ratios of these ions. The so-called Hofmeister series have no real existence, being the result of the fact that the older workers failed to measure the most important variable in the case, namely the hydrogen ion concentration of their protein solutions, a failure for which they can not be blamed since the methods were not sufficiently developed.

II

Pauli and a number of other workers assume that both ions of a neutral salt are adsorbed simultaneously by non-ionized protein molecules. If we consider the hydrogen ion concentration of the proteins we can show

that only the cation or only the anion or that neither ion can combine at one time with a protein; and that it depends solely on the hydrogen ion concentration of the solution which of the three possibilities exists.

Proteins exist in three states, defined by their hydrogen ion concentration, namely, (*a*) as non-ionogenic or isoelectric protein, (*b*) metal proteinate (*e. g.*, Na or Ca proteinate), and (*c*) protein-acid salts (*e. g.*, protein chloride, protein sulfate, etc.). We will use gelatin as an illustration. At one definite hydrogen ion concentration, namely $10^{-4.7}$ N (or in Sörensen's logarithmic symbol at pH = 4.7), gelatin can combine practically with neither anion nor cation of an electrolyte. At a pH > 4.7 it can combine only with cations (forming metal gelatinate, *e. g.*, Na gelatinate), at a pH < 4.7 it combines with anions (forming gelatin chloride, etc.). This was proved in the following way: Doses of 1 gm. of finely powdered commercial gelatin (going through sieve 60 but not through 80), which happened to have a pH of 7.0, were brought to a different hydrogen ion concentration by putting them for 1 hour at about 15° C. into 100 c.c. of HNO_3 solutions varying in concentration from M/8192 to M/8. After this they were put on a filter, the acid being allowed to drain off, and were washed once or twice with 25 c.c. of cold water (of 5° C. or less) to remove remnants of the acid between the granules of the powdered gelatin. These different doses of 1 gm. of gelatin now possessing a different pH were all put for 1 hour into beakers containing the same concentration, *e. g.*, M/64, of silver nitrate at a temperature of 15° C. They were then put on a filter and washed 6 or 8 times each with 25 c.c. of ice cold water; the wash water must be cold since otherwise the particles will coalesce and the washing will be incomplete. This washing serves the purpose of removing the $AgNO_3$ held in solution between the granules, thus allowing us to ascertain where the Ag is in combination with gelatin and where it is not in combination, since the Ag not in combination with gelatin can be removed by the washing while the former can not, or at least only extremely slowly by altering the pH. After having removed the $AgNO_3$ not in combination with gelatin by washing with ice cold water we melt the gelatin by heating to 40° C., adding enough distilled water to bring the volume of each gelatin solution to 100 c.c., determine the pH of each solution potentiometrically or colorimetrically, and expose the solutions in test-tubes to light, the previous manipulations having been carried out in a dark room (with the exception of the determination of pH, for which only part of the gelatin solution was used). In 20 minutes all the gelatin solutions with a pH > 4.7, *i. e.*, from pH 4.8 and above, become opaque and then black, while all the solutions of pH < 4.7, *i. e.*, from 4.6 and below, remain transparent even when exposed to light for months or years. The solutions of pH 4.7 become opaque, but remain white, no matter how long they may have been exposed to light. At this pH—the isoelectric point—gelatin is not in combination with Ag, but it is insoluble. Hence the cation Ag is only in chemical combination with gelatin when the pH is > 4.7 At pH 4.7 or below gelatin is not able to combine with Ag ionogenically. This statement was confirmed by volumetric analysis.

The same tests can be made for any other cation the presence of which can be easily demonstrated. Thus when powdered gelatin of different pH is treated with $NiCl_3$ and the $NiCl_3$ not in combination with gelatin be removed by washing with ice cold water, the presence of Ni can be demonstrated in all gelatin solutions with a pH > 4.7 by using dimethylglyoxime as an indicator. All gelatin solutions of pH of 4.8 or above assume a crimson color upon the addition of dimethylglyoxime, while all the others remain colorless. When we treat gelatin with copper acetate, and wash afterwards, the gelatin is blue and opaque when its pH is 4.8 or above, but is colorless and clear for pH < 4.7. Most striking are the results with basic dyes, *e. g.*, basic fuchsin or neutral red, after sufficient washing with cold water; only those gela-

tin solutions are red whose pH is above 4.7, while the others are colorless.

On the acid side of the isoelectric point, *i. e.*, at pH < 4.7, the gelatin is in combination with the anion of the salt used. This can be demonstrated in the same way by bringing different doses of powdered gelatin to different pH and treating them for one hour with a weak solution of a salt whose anion easily betrays itself, *e. g.*, M/128 $K_4Fe(CN)_6$. If after this treatment the powdered gelatin is washed six times with cold water to remove the $Fe(CN)_6$ not in chemical combination with gelatin and if 1 per cent. solutions of these different samples of gelatin are made, it is found that when the pH is < 4.7 the gelatin solution turns blue after a few days (due to the formation of ferric salt), while solutions of gelatin with a pH of 4.7 or above remain permanently colorless. Hence gelatin enters into chemical combination with the anion $Fe(CN)_6$ only when pH is < 4.7. The same can be demonstrated through the addition of ferric salt when gelatin has been treated with NaCNS, the anion CNS being in combination with gelatin only where the pH is < 4.7. Acid dyes, like acid fuchsin, combine with gelatin only when pH is < 4.7.

In this way it can be shown that when the pH is > 4.7 gelatin can combine only with cations; when pH is < 4.7 it can combine only with anions, while at pH 4.7 (the isoelectric point) it can combine with neither anion nor cation. The idea that both ions influence a protein simultaneously is no longer tenable.

It follows also that a protein solution is not adequately defined by its concentration of protein but that the hydrogen ion concentration must also be known since each protein occurs in three different forms—possibly isomers—according to its hydrogen ion concentration.

In the experiments just discussed it was necessary to wash the powdered gelatin to find out at which pH an ion was in combination with the gelatin. This has led some authors to the belief that in all my experiments the washing was a necessary part of the procedure. I therefore will call especial attention to the fact that the experiments to be described in the rest of the paper were carried out with isoelectric gelatin to which just enough acid or alkali was added to bring it to the hydrogen ion concentration required for the purpose of the experiment.

III

When a protein is in a salt solution, *e. g.*, NaCl, it will combine with Na forming sodium proteinate as soon as the pH is higher than the isoelectric point of the protein; when, however, the pH falls below that of the isoelectric point of the protein the Na is given off and protein chloride is formed.

Moreover, the writer has been able to show by volumetric analysis that the quantity of anion or cation in combination with the protein is an unequivocal function of the pH. When we add HCl to isoelectric gelatin and determine the pH we always find the same amount of Cl in combination with a given mass of originally isoelectric gelatin for the same pH; so that if we know the pH and the concentration of originally isoelectric gelatin present we can also tell how much Cl is in combination with the protein for this pH. The same is true when we add an alkali to the isoelectric gelatin. For the same pH the amount of cation in combination is always the same. These facts have led the writer to propose the following theory. When we add an acid, *e. g.*, HCl, to isoelectric gelatin (or any other isoelectric protein) an equilibrium is established between free HCl, protein chloride, and non-ionogenic or isoelectric protein; when we add alkali an equilibrium is established between metal proteinate, non-ionized protein, and the hydrogen ions. Sörensen was led to a similar view on the basis of entirely different experiments.

IV

This fact that the hydrogen ion concentration of a protein solution determines the quantity of protein salt formed is the basis on which the following proof for the purely chemical character of the combination be-

tween proteins and other bodies rests. The experiments mentioned thus far in this paper do not yet allow us to decide whether the ions are "adsorbed" or in chemical combination with the proteins. We will now show that acids and bases combine with proteins and an alkali. This can be proved in the following way. We know that a weak dibasic or tribasic acid gives off one hydrogen ion more readily than both or all three; while in a strong dibasic acid, like H_2SO_4, both hydrogen ions are held with a sufficiently small

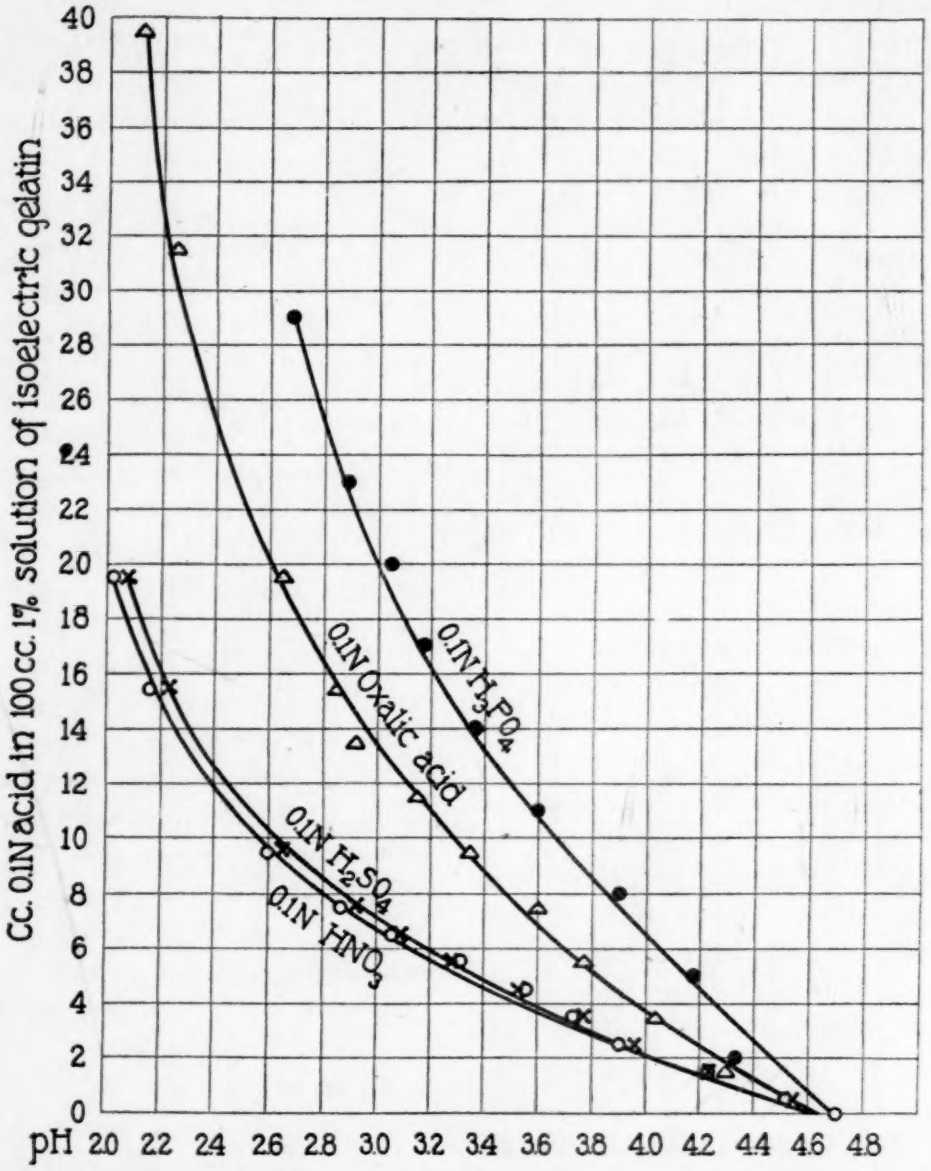

FIG. 1. The ordinates represent the c.c. of 0.1 N acid in 100 c.c. of 1 per cent. solution of isoelectric gelatin required to bring the solution to the pH indicated in the abscissæ. The curves for 0.1 N H_2SO_4 and 0.1 N HNO_3 are identical while the values for H_3PO_4 and oxalic acid differ, being approximately in the ratio of HNO_3: oxalic acid: H_3PO_4 as 1: 2: 3.

in the same way as they combine with crystalline compounds, namely by the purely chemical forces of primary valency. The combination between acids and proteins is analogous to that between acids and NH_3, and the combination between bases and proteins is analogous to that between CH_3COOH electrostatic force to be easily removed. If the forces which determine the reaction between these acids and proteins are purely chemical it would follow that three times as many c.c. of 0.1 NH_3PO_4 are required to bring 100 c.c. of 1 per cent. solution of isoelectric gelatin to a given pH, *e. g.*, 3.0, as are re-

quired in the case of HNO_3 or HCl; while twice as many c.c. of 0.1 N oxalic as of HNO_3 should be required. On the other hand, it should require just as many c.c. of 0.1 N H_2SO_4 as HNO_3 Fig. 1 shows that this is the case. The ordinates of this figure are the c.c. of 0.1 N acid required to bring 1 gm. of isoelectric gelatin to the pH indicated in the abscissae by the four acids mentioned, namely HNO_3, H_2SO_4, oxalic and phosphoric acids. The curves for H_2SO_4 and HNO_3 are identical while, for the same pH, the value for H_3PO_4 is always approximately three times and the value for oxalic acid is always approximately twice as high as for HNO_3.

On the basis of the same reasoning as applied to acids we should expect that equal numbers of c.c. of 0.1 N $Ca(OH)_2$ and $Ba(OH)_2$ as of LiOH, NaOH, and KOH should be required to bring 100 c.c. of a 1 per cent. solution of isoelectric gelatin to the same pH and the writer was able to show that this is the case. Similar results were obtained with crystalline egg albumin.

When we have a solution of a gelatin-acid salt of originally 1 per cent. isoelectric gelatin and of a certain pH, *e. g.*, 3.0, we have free acid in the solution and a certain amount of the anion of the acid in combination with gelatin. We can find out by volumetric analysis how much of the anion is in combination with the protein by making certain corrections discussed in former papers. In this way it can also be ascertained that all weak dibasic acids combine in molecular proportions with isoelectric protein, while strong dibasic acids and diacidic alkalies combine in equivalent proportions with proteins, as is shown by Table I. It follows from this table that for the same pH the amount of HNO_3, oxalic, and phosphoric acids in combination with the same quantity of originally isoelectric gelatin is always in the proportion of 1:2:3.

TABLE I

C.c. of 0.01 N Acid in Combination with 10 c.c. of a 1 Per Cent. Gelatin Solution at Different pH

pH	3.1	3.2	3.3	3.4	3.5	3.7	3.9	4.1	4.2	4.3
HNO_3 ...	4.35	4.1	3.6	3.2	2.85	2.45	1.9	1.45		0.75
Oxalic acid	9.6	8.75	7.6	6.7	6.00	4.3	3.0		1.65	
H_3PO_4 ...		12.4	10.4	9.8	9.00	7.4	5.8	4.5	2.6	2.1

We can therefore state that the ratios in which ions combine with proteins are identical with the ratios in which the same ions combine with crystalloids. Or in other words, the forces by which gelatin and egg albumin (and probably proteins in general) combine with acids or alkalies are the purely chemical forces of primary valency.

V.

The most important fact for our purpose is that from the combining ratios just mentioned the influence of acids and bases on the physical properties of proteins can be predicted. This influence is altogether different from that stated in the so-called Hofmeister series of ions or by the ion series of Pauli and his collaborators, and this difference is due to the fact that these latter authors compared the effects of equal quantities of acids or alkalies while we found it necessary to compare the physical properties of solutions of proteins of the same hydrogen ion concentration. If this is done the following rule is found. All those acids whose anion combines as a monovalent ion raise the osmotic pressure, viscosity, swelling of protein about twice as much as the acids whose anion combines as a bivalent anion for the same pH. The same valency rule holds for the cations of different alkalies.

We have seen that at the same pH three times as many c.c. of 0.1 N H_3PO_4 as of HNO_3 are in combination with 1 gm. of originally isoelectric gelatin in 100 c.c. of solution. It follows from this that the anion of gelatin phosphate is the monovalent ion H_2PO_4 and not the trivalent anion PO_4. It follows likewise from the combining ratios discussed that the anion of oxalic acid in combination with protein is the monovalent anion HC_2O_4. The same is true for all weak dibasic or tribasic acids, namely that they combine with proteins forming protein salts with monovalent anion. It follows also from the combining

ratios that the salt of a protein with a strong dibasic acid, as H_2SO_4, however, must have a divalent anion, *e. g.*, SO_4. If we compare the viscosity or osmotic pressure of 1 per cent. solutions of originally isoelectric gelatin with different acids of the same pH we find that these properties are identical for all gelatin salts with monovalent anion; in other words, 1 per cent. solutions of gelatin chloride, bromide, nitrate, tartrate, succinate, citrate, or phosphate have all the same vis-

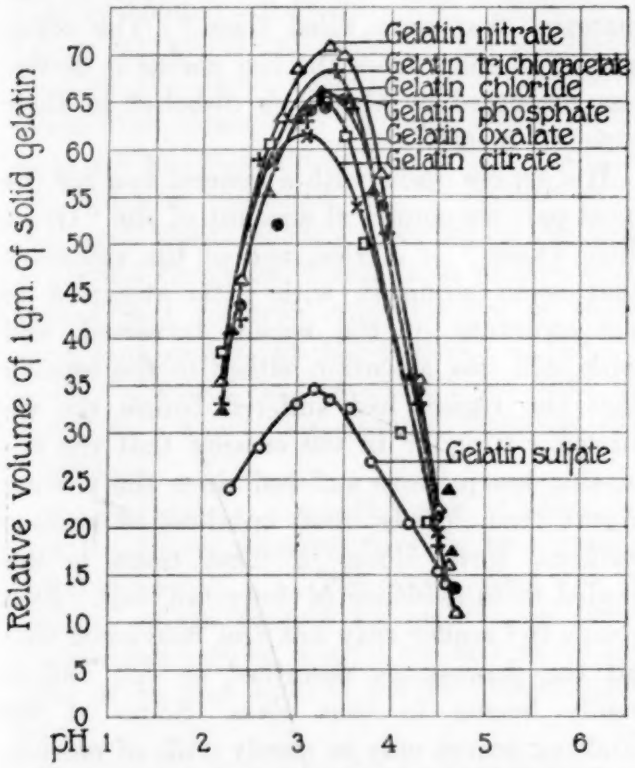

FIG. 2. Influence of different acids upon the swelling of gelatin when plotted over pH as abscissæ. The curves show that nitric, trichloracetic, hydrochloric, phosphoric, oxalic, and citric acids cause approximately the same degree of swelling, while sulfuric acid causes only about one half the amount of swelling. In the case of gelatin sulfate the anion is divalent; in the case of the other acids used it is monovalent. According to the Hofmeister series the curves for phosphate, oxalate and citrate should coincide with that of sulfate instead of coinciding with that of chloride.

cosity, and the same osmotic pressure at the same pH. The same is true for the swelling (Fig. 2). If we plot the curves for these three properties with pH as abscissæ and the values for osmotic pressure, viscosity, and swelling as ordinates, we get practically identical curves for gelatin chloride, bromide, nitrate, tartrate, succinate, citrate, and phosphate. The values for swelling are a minimum at pH 4.7 (the isoelectric point of gelatin) they rise rapidly with the fall of pH until they reach a maximum at pH about 3.2, and then they drop again. Each curve is the expression of an individual experiment. The maximum in the curves for gelatin chloride, bromide, nitrate, tartrate, succinate, citrate and phosphate is practically identical, the variations between the values for these acids lying within the limit of variation which we may expect if we plot six different experiments with the same acid. When, however, we plot the same curves for gelatin sulfate, we get curves which are considerably lower, reaching a height of only one half (or a little less than) those of gelatin-acid salts with monovalent anions. It may be of interest to compare our curves with those expected on the basis of Pauli's and Hofmeister's ion series. According to the latter theory the curves for phosphates, oxalates, citrates, and tartrates should be in the region of the SO_4 curve but not in the region of the Cl curve. Those authors who observed such differences did not measure the hydrogen ion concentration, attributing the effects due to the difference in the hydrogen ion concentration of their gelatin solutions erroneously to a difference in the anion effect. These elementary errors form the basis of a number of speculations current in biology and pathology.

When we compare monobasic acids of different strength, *e. g.*, acetic, mono-, di-, and trichloracetic acids, we find that the weaker the acid the more acid must be contained in a 1 per cent. solution of originally isoelectric gelatin to bring it to the same pH. If we compare the effect of these four acids on the osmotic presure of gelatin we find that it is (within the limits of accuracy of these experiments) identical for the same pH. The curves for the influence of these four acids on the osmotic pressure of gelatin solution are practically identical when plotted over the pH as abscissæ; and, moreover, the curves

are identical with the curves for HCl or H_3PO_4 in Fig. 1. The explanation of this fact is that at the same pH the same mass of originally isoelectric gelatin is in combination with the same quantity of these four acids and since the anions of these four acids are all monovalent the curves must be identical.

As far as the alkalies are concerned, we notice that the curve representing the effect of the weak base NH_4OH on the physical properties of proteins is the same as that for the strong bases LiOH, NaOH, KOH when plotted over pH as abscissæ, while the curves representing the effect of $Ca(OH)_2$ or $Ba(OH)_2$ on the same properties are considerably lower.

It is obvious that the valency of the ion in combination with the protein has a noticeable influence on the properties of the protein salt formed, while the protein salts with ions of the same valency have all the same properties. The fact of the greatest importance is, however, that the influence of acids and bases on the physical properties of proteins is the expression of the combining ratios of the acids or bases with proteins so that we are able to predict the value of the physical properties from the combining ratios. This fact seems to give a final decision in favor of a purely chemical theory of these influences and against the colloidal theories as based on the Hofmeister or Pauli ion series.

The behavior of the proteins therefore contradicts the idea that the chemistry of colloids differs from the chemistry of crystalloids.

Jacques Loeb

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THE AFRICAN RIFT VALLEYS

A recent article[1] with the above title by Professor J. W. Gregory, of Glasgow, is of interest from the general summary that it presents of a remarkable group of natural features, as well as from the ingenious flight of geological imagination by which it explains them. The term, rift valley, introduced by Gregory in connection with his African studies of 25 years ago, designates a longitudinal depression "caused by the material sinking in mass, so that what is now its floor formerly stood level with the highlands on each side." Such valleys therefore contrast strongly with "ordinary valleys, which are caused by the removal piecemeal by rivers or wind of the material that once filled them." The omission of glaciers from the last clause is doubtless prompted by Gregory's disbelief in their capacity to erode.

His article opens with a general and for the most part an empirical account of the "Great Rift Valley" of Africa, and of the volcanoes that occur along it, with little attention to the structure of the region traversed, and with still less attention either to the erosion that the region had suffered before the assumed rifting or to the erosion that the enclosing scarps have suffered since the rifting. Apart from briefly cited opinions of various authors, form alone in most cases is appealed to in evidence of down-faulting. As a result the reader may not feel convinced that all the depressions described as rift valleys really belong in that class. Some of the limiting scarps may be purely cliffs of erosion. Indeed, the inclusion of the Red Sea, with the narrow Gulfs of Suez and of Akaba at its northwest end and the broad Gulf of Aden to the southeast of it, as rift valleys, and the drawing of several "diagonal tectonic lines" along certain parts of the east African coast that are supposed to have been "cut off by . . . faulting" suggest so open a hospitality to the occurrence of rifts and rift valleys as to make the reader wonder whether they are not overworked. The first part of the article is therefore chiefly valuable as a topographic summary of a remarkable region. A critical discussion of the evidence for rifting, based on a review of the many articles cited in the bibliography, would make an excellent subject for an advanced student in physiography.

[1] "The African Rift Valleys," by J. W. Gregory, *Geogr. Jour.*, LVI., 1920, 13–47, with 6 maps, 7 profiles and 8 half-tone plates, and a bibliography of 65 titles.

The article, although published in a journal from which geological discussions are usually excluded, devotes its second part to a strictly geological discussion of the "age and history" of the African rift valley, with the result of assigning it a relatively modern date. The article closes with an inquiry into the orgin of the rift valley and its relation to contemporary earth movements; and here speculation is given free rein. The following extracts are from the second and closing parts: running comments are added. The length of the valley is "about one sixth of the circumference of the earth. It must have had some world-wide cause." It is "dependent in the main on the volcanic history of the country; the two are connected, as the subsidence of the earth blocks doubtless forced up the lavas along the fractures;" and from this it would appear that volcanic eruptions are to be regarded as caused by the sinking of the rift blocks, although the opposite view, that the eruptions caused the sinking, deserves consideration.

> The first stage . . . was the uplift of a long, low arch [by lateral pressure] with the axis trending north and south. . . . The second stage was the cracking of the sides of the arch as the lateral pressure was reduced, and the top sank as the keystone of an arch sinks if the end supports give way.

But it may be questioned whether there is any true homology between the crest of a very broad and low earth-crust anticline with plenty of subcrustal material crushed up beneath it, and the keystone of an arch of masonry which spans an open space; and moreover the lateral pressure here assumed contradicts the state of tension, mentioned below. "The sinking of the keystones . . . into the plastic material below forced some of it up the adjacent cracks, through which it was discharged in volcanic eruptions." The possibility that the primary impulse was not lateral pressure in the superficial crust, but an upward pressure of deep-seated and crowded lavas is not considered.

The speculative character of the views here exposed reaches its climax on the closing page:

> The essentially different character of the contemporary earth-movements of Africa and of the Pacific borders is explained by their antipodal position. Africa was antipodal to the Pacific, and it is in accordance with the well-known antipodal relation of ocean to continent that while the Pacific was sinking and the crust beneath it undergoing compression, its antipodal land should be rising and subject to tension. . . . The subsidence of the Arabian sea and the outflow of the vast quantities of lava to the east and west [in India and Abyssinia] left the east African arch insufficiently supported, and the top of it sank between parallel fractures. . . . Africa was in tension, and torn by north and south fractures, along which the sinking of a strip of the crust formed the longest meridional land valley on the earth. . . . There are two great regional disturbances in the Eastern Hemisphere with which they [the rift-valley faults] may be correlated; the foundering of the Indian Ocean . . . and the movements which raised the Alpine-Himalayan Mountains. . . . The Great Rift Valley . . . owes its unique character to its position antipodal to the Pacific, and its course to the wrench in the crust of the Eastern Hemisphere between the segment pressing northward against Europe and that pressing southward in Asia toward the deepening basin of the Indian Ocean.

These are truly far-reaching generalizations, but whether they will endure is uncertain. If they are correct their prophetic power is great, for the evidence to demonstrate them is not yet fully collected.

The doubts that are aroused by a reading of Gregory's article are re-enforced by the protest published a month later by Ball, of the Geological Survey of Egypt.[2] He points out that, as large sections of the supposed rift valleys are submerged in the Red Sea and the Gulf of Aden, and as not a hundredth part of the rest has been examined by competent geologists, much uncertainty must still remain as to their structural interpretation and physical origin. He adds that, as a result of Gregory's early work on the subject, some of

[2] "The African Rift Valleys," by John Ball *Geogr. Jour.*, LVI., 1920, 234–238.

the members of the Egyptian Geological Survey "became obsessed by the notion that the Nile Valley and the Gulf of Suez were parts of the rift system," and that a later interpretation of both these features as valleys of erosion—one of them moderately submerged—was much retarded by that premature conclusion. He rejects Gregory's correlation of the Pacific basin and the African plateau, doubts the production of rift valleys by tension, suggests that their structure may be due to compression, and notes that their form may have been much influenced by erosion, especially in those African depressions which have a scarp only on one side. He therefore wisely urges that the whole problem should be regarded as needing much further investigation before a safe conclusion can be announced.

Ball is, however, over-conservative in asserting that "the only sure proof of the existence of a trough fault is to be obtained by tracing across the floor of the trough the same strata as occur at higher levels in the bounding scarps." This overlooks the competent physiographic evidence of faulting that is provided when a nearly rectilinear scarp truncates a series of deformed strata or a body of massive rocks, as pointed out by Gilbert in his studies of the Great Basin ranges. For example, the trough of the Rhine from Basal to Bingen rarely shows rock outcrops on its floor; evidence of its depression between subparallel faults is derived chiefly from the topography of its enclosing scarps. Similarly, the Limagne, a broad rift-depression in central France, drained northward by the Allier between the highlands of Auvergne on the west and those of the Monts du Forez on the east, is floored with lacustrine or fluviatile sediments of modern date, while the enclosing highlands consist of ancient crystalline rocks. The existence of marginal faults here has long been accepted by French geologists, although the kind of evidence dmanded for faults by Ball is not forthcoming. Crystalline rocks are not seen on the floor of the depression, and even if they were, they could not be proved to be down-faulted. The evidence of down-faulting is found in the nature of the enclosing scarps. It may be noted in passing that the Limagne is not strictly a rift valley, but a resequent rift valley, in that since its first production by down-faulting, when it was truly a rift valley, it has been filled with inwashed sediments to the level of the enclosing highlands at a time when the whole region stood lower than now, and later on, after a broad elevation without faulting, the inwashed sediments have been washed out as deep as river-grade permits, thus again leaving the enclosing scarps in relief. The evidence of this succession of events is clearly furnished by the presence of isolated volcanic necks in the midst of the depression, and of surviving spurs of the weak sediments capped with lava flows, that project into the depression about at the level of the adjacent highlands. A resequent rift-valley of this kind must evidently differ from an initial rift-valley in having the height of its enclosing scarps determined by the depth of recent erosion, not by the drop of the original faulting.

To return to Africa: If so great a series of rift valleys really exists there as is represented on Gregory's map, some of them should show scarps that truncate the structures of the enclosing highlands, and the evidence that such scarps provide for down-faulting should not be overlooked. The possibility that some of the African rift valleys have been filled and excavated again in resequent fashion like the Limagne should also be inquired into.

W. M. Davis

Cambridge, Mass.,
October 31, 1920

SCIENTIFIC EVENTS

THE SIXTEENTH ANNUAL NEW ENGLAND INTERCOLLEGIATE GEOLOGICAL EXCURSION

The sixteenth excursion of the New England geologists was taken in the vicinity of Middletown, Connecticut, October 8 and 9, under the direction of Professors William North Rice and Wilbur G. Foye. About twenty-five persons were in attendance, among whom were representatives from Harvard, Massachusetts Agricultural College, Mount

Holyoke, Smith, Trinity, University of Vermont, Wesleyan, Williams and Yale.

On Friday afternoon, October 8, the pegmatite dikes at Collins Hill, Portland, were visited. Twenty-two mineral species have been found at this locality and the party was fortunate in collecting, among other species, flat, purple, transparent apatite crystals which showed strongly developed facets of the first and second order prisms, the first and second order pyramids, and the basal pinacoid. The relations of the pegmatites to the Bolton schist, the intrusive contacts of the Maromas and Glastonbury gneisses with the schist, the interglacial course of the Connecticut river were brought to the attention of the party.

Friday evening an excellent buffet lunch was served at Fish Hall by invitation of Wesleyan University. After the lunch the party listened to an address by Professor W. M. Davis, whose classic work on the complicated structure of the Connecticut Triassic has long been a model for structural geologists. During the lecture he gave a most interesting account of the methods he employed in working out the fault structures in the vicinity of the excursion of the following day. He also discussed the mechanics of the faulting and erosion which produced the striking topography of the Connecticut valley.

At the conclusion of this lecture, Professor Rice, also a pioneer student of the Connecticut Triassic, described in detail the faulted structures between the Lamentation Mountain and the Hanging Hills blocks which were to be visited on the morrow. Professor Foye exhibited a collection of minerals from the pegmatite dikes in the vicinity of Middletown, and gave a brief account of the localities from which they were obtained.

Saturday morning the party was conducted by autotruck and on foot to some of the step faults and drag dips along the line of the great fault described by Professor Rice the evening before.

Lunch was eaten at Spruce brook by a picturesque waterfall. After lunch the resignation of the secretary who had been in office for eighteen years was accepted, and Professor Foye was elected for an indefinite term.

At Spruce Brook the contact of the main trap sheet with the overlying conglomerate was studied. Pebbles of the underlying trap were found in the basal layers of the conglomerate and the contemporaneous character of the main sheet was established.

At two localities, lying west of Lamentation Mountain, the problematic "pillow lavas" of the anterior sheet were examined, and their origin discussed by members of the party. The Meriden "ash bed," also within the anterior sheet, was visited and created considerable discussion, but the general opinion favored a volcanic source for the deposit.

At the Lane quarry north of Meriden was seen a pahoehoe surface of lava overlain by a denser flow showing that, in this locality at least, the main trap sheet did not consist of but a single flow.

None of the many enjoyable and profitable excursions taken by the New England geologists has been more successful than this one.

HERDMAN F. CLELAND,
Retiring Secretary

"PHYSIOLOGICAL REVIEWS"

A NEW journal under this name will be published quarterly by the American Physiological Society under the editorial direction of W. H. Howell, Baltimore; Reid Hunt, Boston; F. S. Lee, New York; J. J. R. Macleod, Toronto; Lafayette B. Mendel, New Haven; H. Gideon Wells, Chicago; D. R. Hooker, Managing Editor, Baltimore.

The main purpose of the *Physiological Reviews* is to furnish a means whereby those interested in the physiological sciences may keep in touch with contemporary research. The literature, as every worker knows, is so extensive and scattered that even the specialist may fail to maintain contact with the advance along different lines of his subject. The obvious method of meeting such a situation is to provide articles from time to time in which the more recent literature is compared and summarized. The abstract journals render valuable assistance by condensing and classifying

the literature of individual papers, but their function does not extend to a comparative analysis of results and methods. Publications such as the *Ergebnisse der Physiologie,* the Harvey Lectures, etc., that attempt this latter task, have been so helpful as to encourage the belief that a further enlargement of such agencies will be welcomed by all workers. It is proposed, therefore, to establish a journal in which there will be published a series of short but comprehensive articles dealing with the recent literature in physiology, using this term in a broad sense to include bio-chemistry, biophysics, experimental pharmacology and experimental pathology.

The editorial board will select subjects and assign them to authors. The articles will contain complete bibliographical lists and will be as short as the material under treatment will allow. Thus it is estimated that the first volume will contain twenty articles averaging twenty-five pages each. The character and scope of these articles may be judged by the contents of Volume I, which are planned to be as follows:

JANUARY

The regulation of the pulmonary circulation: CARL J. WIGGERS.
The origin and propagation of the cardiac impulse: J. A. E. EYSTER and W. J. MEEK.
The anaphylactic reaction: H. GIDEON WELLS.
Photo-electric current in the eye: CHARLES SHEARD.
The carbon dioxide carrier of the blood: DONALD D. VAN SLYKE.

APRIL

Blood volume and its regulation: JOSEPH ERLANGER.
The sugar of the blood: J. J. R. MACLEOD.
The circulation in the capillaries and veins: DONALD R. HOOKER.
The heat regulating mechanism of the body: HENRY G. BARBOUR.
Contributions of war surgery to the physiology of the (central) nervous system: GILBERT HORRAX.

JULY

Structure and significance of the phosphatids: P. A. LEVENE.
Physiological oxidations: H. D. DAKIN.
Tests for muscular efficiency: E. G. MARTIN.
Intestinal absorption: SAMUEL GOLDSCHMIDT.
Gastric secretion in health and disease: A. J. CARLSTON.

OCTOBER

The afferent paths for the visceral reflexes: S. WALTER RANSON.
The physiological effects of undernutrition: GRAHAM LUSK.
Adsorption in physiological processes: ALBERT P. MATHEWS.
The vitamines: H. C. SHERMAN.
Physiological effects of altitude: EDWARD C. SCHNEIDER.

UNIVERSITY OF PENNSYLVANIA LECTURES

TWENTY-FIVE lectures by members of the faculty of the University of Pennsylvania will constitute the free lecture program for the present academic year. All lectures will be given in Houston Hall. The lectures on scientific subjects are as follows:

Nov. 13—"Franklin's life in title pages," Asa Don Dickinson.

Nov. 27—"New light on plant feeding," Rodney H. True, Ph.D.

Dec. 4—"Some applications of biochemistry," George H. Meeker, M.D., LL.D.

Jan. 22—"Engineering and science," Milo S. Ketchum, B.S.

Feb. 12—"Bessemer steel first invented in America," Herman C. Berry, B.S.

Feb. 19—"Death from the standpoint of the physiologist," Edward Lodholz, M.D.

March 5—"Some important phases of public health," Henry R. M. Landis, M.D.

March 12—"Floral calendars and floral clocks," John M. Macfarlane, D.Sc.

March 19—"The criminal prosecution of animals and inanimate objects," Walter W. Hyde, Ph.D.

March 26—"Electrical transmission of power," Harold Pender, Ph.D.

April 2—"The relation of bacteria to health and disease," David H. Berger, Dr.P.H.

April 9—"Methods of determining celestial distances," Samuel G. Barton, Ph.D.

April 16—"The elimination of vibration and noise," Thomas D. Cope, Ph.D.

April 30—"The advances of medicine in the nineteenth century," Alfred Stengel, M.D., Sc.D.

May 7—"Louis Pasteur: the world's greatest benefactor," Ernest Laplace, M.D., LL.D.

THE SECTION OF ZOOLOGY OF THE AMERICAN ASSOCIATION

THE Convocation Week meetings of Section F (Zoology) of the American Association for the Advancement of Science will be held in conjunction with those of the American Society of Zoologists at Chicago University, Chicago, Illinois, December 28, 29 and 30. As the officers of the American Society for Zoologists are responsible for the program under the rules of the American Association, all titles and abstracts of papers should be sent to Professor W. C. Allee, Lake Forest College, Lake Forest, Illinois.

The address of the retiring vice-president of Section F, Professor William Morton Wheeler, will be given at the Zoologists Smoker, Tuesday evening, December 28.

H. V. NEAL,
Secretary of Section F
TUFTS COLLEGE, MASS.

SCIENTIFIC NOTES AND NEWS

DR. SAMUEL JAMES MELTZER member of the Rockefeller Institute for Medical Research and head of the department of physiology and pharmacology, died on November 7 at the age of sixty-nine years.

THE address of Dr. Simon Flexner, as retiring president of the American Association for the Advancement of Science, will be given at the Chicago meeting on the evening of Monday, December 27, instead of on Tuesday evening as originally planned.

SIR ALMROTH WRIGHT, has received the first award of a gold medal established through the gift of an anonymous fellow of the Royal Society of Medicine and open to medical practitioners throughout the world.

AT the conclusion of the Harveian Oration, delivered by Sir Frederick Andrewes on October 18, before the Royal College of Physicians of London, the President, Sir Norman Moore, presented the triennial Bisset Hawkins memorial medal for distinction in public health to Dr. W. H. Hamer, medical officer of health to the London County Council.

DR. WALDEMAR T. SCHALLER has severed his connection with the Great Southern Sulphur Co., Inc., of New Orleans, La., and has returned to the U. S. Geological Survey, Washington, D. C.

MR. H. H. BROWN, formerly connected with the Bureau of Chemistry, Department of Agriculture, where he was in charge of the chemical investigations on the cause and prevention of dust explosions, is at present employed by the Pejepscot Paper Co., Brunswick, Maine, to establish a chemical research laboratory and to investigate chemical problems connected with the manufacture of paper and the utilization of waste products.

AT a meeting of the Institute of Medicine of Chicago, October 29, Professor Graham Lusk, New York, delivered the first Pasteur Lecture on "Some influences of French science on medicine."

DR. VICTOR G. HEISER, director for the East of the International Health Board, recently delivered a lecture at the School of Hygiene and Public Health of the Johns Hopkins Hospital on the work done by the United States government in the betterment of health conditions in its dependencies.

MR. ELMER A. SPERRY, president of the Sperry Gyroscope Company, of New York, is to give a demonstration lecture at the Harvard Union, November 15, at 8 P.M., on "Applications of the gyroscope to navigation." The lecture is held under the auspices of the department of astronomy at Harvard University, and will be open to the public.

THE eleventh course of lectures under the Herter Foundation was delivered by Dr. Jules Bordet, director of the Pasteur Institute, Brussels, in the Johns Hopkins Hospital, on October 26, 27 and 28.

THE Emil Fischer memorial lecture was delivered by Dr. M. O. Forster at the meeting of the Chemical Society, London, October 28.

MEMORIAL exercises for the late Dr. Samuel Sheldon, professor of physics and electrical engineering at the Polytechnic Institute of Brooklyn, will be held in the auditorium of

the Engineering Societies Building in New York City on the evening of November 17. Addresses will be made by Dr. Arthur E. Kennelly, professor of electrical engineering at Harvard University; Dr. William H. Nichols, chairman of the corporation of the Polytechnic Institute; Mr. T. Commerford Martin, secretary of the National Electric Light Association; Mr. Bancroft Gherardi, vice-president of the American Telephone and Telegraph Co., and William N. Dickinson, president of the New York Electrical Society. Mr. Arthur W. Berresford, president of the American Institute of Electrical Engineers, will preside.

M. Louis du Hauron, a pioneer in the photography of color, died on August 31, at the age of eighty-three years.

The death has occurred on the way to New York of Dr. Manuel C. Barrios, professor of physiology and later of legal medicine at the University of Lima, Peru. While a member of the national cabinet he organized the public health service and founded the National Academy of Medicine.

Dr. J. P. Morat, formerly professor of physiology at the University of Lyons, has died at the age of seventy-five years.

The Berlin correspondent of the *Journal* of the American Medical Association writes that the first meeting in seven years of the Association of German Scientific Men and Physicians, held at Bad Nauheim, was attended by 2,600 members. Among the principal subjects of interest of the meeting were the newer researches in physical science, including the structure of molecules and atoms, and the Einstein theory of relativity. The papers of Dr. Max Rubner, Berlin, and Dr. M. von Gruber, Munich, on the problem of nutrition were probably the outstanding medical contributions.

The Swedish government has approved the plan to found an institute for research on moral assistance for the furtherance of medical faculty to plan the institution. A committee from the faculty has been appointed for the purpose, comprising Drs. Lennmalm, Johansson, Müller and Gadelius, of the chairs of neurology, physiology, anatomy and psychiatry, respectively.

The will of the late Dr. Lloyd Roberts, of Manchester, contains the following bequests: To the Rylands Library, Manchester, such of his bound books as the trustees may select; to the Corporation of Manchester, for the Art Gallery, such of the mezzotints, water-colors, and paintings as they may desire; to the Royal Society of Medicine £5,000; to St. Mary's Hospital, Manchester, £5,000; to Manchester Royal Infirmary, £3,000; to the Royal College of Physicians, London, £3,000; to the Medical Society of London, £2,000; to St. David's College, South Wales, £2,000. The residue of his estate is left to University College, Bangor, with provision that £5,000 be set aside to found a Lloyd Roberts professorship in any subject the authorities think fit.

Professor A. F. Rogers, of Stanford University, is engaged in a mineralogical study of fossil bone and will welcome any specimens that may be sent to him. Small specimens, 2 or 3 inches in size, will suffice. They may be sent by express, collect to the Mineralogy Laboratory (care of A. J. Rogers), Stanford University, California.

There has recently been formed at Brussels a Fédération belge des sociétés des sciences mathématiques, physiques, naturelles médicales et appliquées. According to the *Journal* of the American Medical Association thirty-three societies have already joined the federation, and many others have promised to become affiliated in the near future. The group includes various scientific societies which give evidence of their activity by the publication of original work, and the movement is designed to unite and coordinate all efforts for the general advancement of the pure and applied sciences, although the component societies retain their own autonomy. The federation will endeavor to encourage initiative and to procure the requisite material and moral assistance for the furtherance of scientific work, and it will collaborate in perfecting scientific publications. It may, especially, summon congresses, organize confer-

ences and expositions, and found or support useful scientific institutions. Among the principal aims of the federation may be mentioned: (1) an increase of the resources of affiliated societies sufficient to continue their publications, and (2) the collection of works published by the federated societies and exchange of publications with analogous federations in allied and neutral countries. A general council of fifteen members administers the affairs of the federation. The officers for 1920 are Professor de la Vallée-Poussin (Louvain), president; Paul Pelseneer (secretary of the Académie royale de Belgique), vice president; J. Wauters (secretary general of the Société chimique de Belgique) treasurer, and Messrs, Zunc and Lucien, secretaries.

UNIVERSITY AND EDUCATIONAL NEWS

THE Hudson's Bay Company, as one means of celebrating the 250th anniversary of its foundation and its long connection with Western Canada and with Winnipeg, has given the University of Manitoba a fellowship of the annual value of $1,500 for the years 1920–29 inclusive, open to graduates of any Canadian university. Each fellow will devote his entire time to original research in some branch of pure or applied science.

A LABORATORY for research on dyestuffs and explosives has been established at George Washington University, under the general supervision of Professor H. C. McNeil, and in charge of Mr. G. W. Phillips, formerly of the Chemical Warfare Service. Dr. Charles E. Munroe will be consulting chemist of the laboratory.

DR. S. A. MAHOOD, of the Forest Products Laboratory, Madison, will have charge of chemistry at Tulane University.

DR. JAMES W. PAPEZ, professor of anatomy and neurology at Emory University School of Medicine, Atlanta, Ga., has resigned to accept the assistant professorship of neurology at Cornell University Medical College, Ithaca, New York.

DR. KENNETH D. BLACKFAN, associate professor of pediatrics at the Johns Hopkins Medical School, has been appointed professor of pediatrics at the Medical College of the University of Cincinnati.

OLAF P. JENKINS has returned to Pullman, Washington, as assistant professor of economic geology in the State College of Washington, having spent the last year with the Sinclair Exploration Company as chief geologist in Alabama.

DR. C. NUSBAUM, formerly of the Magnetic Section of the Bureau of Standards, has been appointed research associate in the division of industrial research and cooperation of the Massachusetts Institute of Technology.

DR. HENRY W. STAGER, for many years head of the department of mathematics in Fresno Junior College, Fresno, California, and more recently with the United States Railroad Administration, has been appointed instructor of mathematics in the University of Washington, Seattle, Washington.

MR. W. VERNON GODSHALL, formerly assistant professor of chemistry at Miami University, has accepted a similar position in physiological chemistry in the department of physiology of the University of Wisconsin. He is also chemist on the Interdepartmental Board for the estimation of the elimination of arsenic from patients treated with arsenical drugs.

AT the Montana School of Mines Assistant Professor Gerald S. Lambert, of Leland Stanford University, has been appointed associate professor of geology, and Dr. A. E. Koenig, assistant professor of chemistry at the University of Wisconsin, associate professor of chemistry.

DR. RODNEY B. HARVEY has resigned as plant physiologist, Bureau of Plant Industry, Washington, D. C., to accept the position of assistant professor in plant physiology at the University of Minnesota, and assistant plant physiologist in the Minnesota Experiment Station.

MR. F. B. SMITH, who recently retired from the position of secretary of agriculture to the

Union of South Africa, has been appointed a reader in estate management at Cambridge

DISCUSSION AND CORRESPONDENCE

THE PRESERVATION OF WILD LIFE

The Ecological Society of America's committee on the preservation of natural conditions, while unable to deal with problems concerning wild life not in reserves, continually encounters the fact that individual species are menaced with extinction by agricultural encroachments. Two of these menaces are:

1. Clean-culture (roadside mowing and burning) as distinguished from roadside and streamside shrubbery and bird and original life preservation.

Birds are decreasing for lack of nesting sites, on account of destruction of breeding conditions. Entomologists and some agriculturists maintain that this condition is necessary to agriculture. Bird men insist that birds are also essential. It is known that a few states encourage roadside shrubbery while several require roadside mowing. The practise in the various parts of the United States and Canada should be ascertained. The effect of different procedures should be determined. The areas in which specially destructive and drastic measures such as burning for insect pests are necessary should be clearly defined and limited and the public informed as to the dangers of such burning.

2. Upland marshes are important as sponges storing water and letting it out slowly during dry seasons, thus controlling floods. Such marshes are gradually being drained and the flood menace is increasing every year.

The only way to save these natural resources and at the same time, the swamp faunas, especially the birds, is to utilize the swamps for aquiculture. To this end several water-culture experiment stations should be established. For the present there should be one, perhaps at Cornell University, to deal with the upland marsh problems. There should be another in connection with Okefinokee swamp and one in connection with the coastal swamps of New Jersey. In addition to frogs, fish, and birds, a number of plants are good for food, etc.; *e. g.*, cattail flour and cattail paper have recently been tried with success. Swamp potatoes, the corns of arrowhead, and seeds, roots, and stalks of our native lotus served as food for the American aborigines and pioneers. Hedrick (Science, 40:611), Claussen (*Sci. Mo.*, 9:179), and Needham and Lloyd ("Life of Inland Waters") have discussed these questions and suggested or advocated the improvement and culture of aquatic plants.

It is the belief of the committee that all organizations in any way interested should combine efforts for the investigation of these questions.

For a list of the committee members, see Science, March 26, 1920; since that date the following have been added: Z. P. Metcalf, University of North Carolina; C. A. Shull, University of Kentucky; R. M. Harper, College Point, N. Y.; and Jens Jensen, Ravinia, Illinois.

V. E. Shelford,
Chairman

University of Illinois

PREDILECTION AND SAMPLING OF HUMAN HEIGHTS

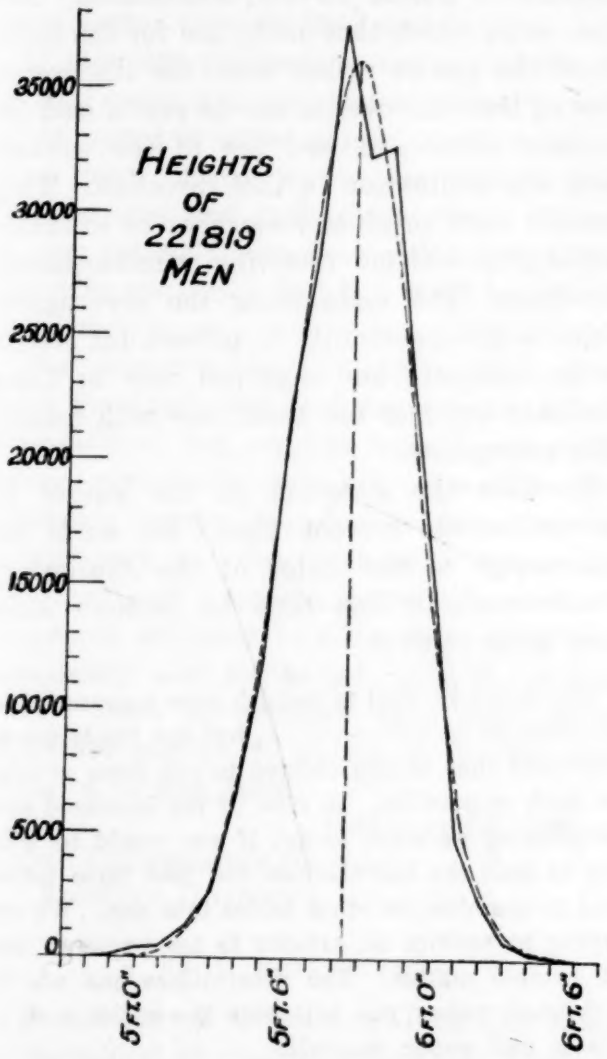
To the Editor of Science: Extensive reliable data showing the distribution of human heights in "unselected" populations are surprisingly hard to obtain. The Association of Life Insurance Medical Directors and the Actuarial Society of America have, however, undertaken a very careful statistical study of men accepted for life insurance,[1] which provides, among other things, a distribution of the heights of 221,819 men. Here, at last, we might expect to settle the question of the form of distribution that would hold for a population, but we discover in the distribution curve a remarkable inversion that it is difficult to explain as anything other than an artefact.

This distribution curve is the solid line of the figure. The average height is 5 ft. 8.49 in. Since the curve is plotted in units of an inch,

[1] "Medico-Actuarial Mortality Investigation," Vol. I., 1912, esp. 11–22.

the peak is at 5 ft. 8 in. The inversion occurs just above the peak: there are fewer men recorded at 5 ft. 9 in. than at 5 ft. 10 in. That the inversion is not due to the inclusion of men of different ages in the same curve is shown by the fact that it occurs for ten of thirteen age-groups (five-year range) taken separately.

The question arises as to the cause of this striking feature of the curve.



We are at liberty to conclude that the observed curve is the "true" distribution of American male heights, but most persons, whether they believe in the *a priori* necessity of the Gaussian form of distribution or not, will be under a strong conviction that the curve should at least be smooth and not bimodal.

The alternative explanation is that the inversion is an artefact. The sampling of the records in the offices of the insurance companies was carefully done. All risks accepted during January of the odd years and July of the even years throughout a sixteen-year period were included. It would seem probable, therefore, that the selection must have occurred when the measurements were made. Many persons who were nearest 5 ft. 9 in. must have been recorded as 5 ft. 8 in. or 5 ft. 10 in. Here we would have a case of artificial selection that depends on the factors involved in obtaining measurements, a case analogous to the predilection that occurs when estimates are made in "round numbers" or, as happened in this investigation, to the predilection to give weights as multiples of five pounds.[2] We may hazard that the error occurred not so much in reading the measuring stick as in the acceptance by the examining physician of the person's own statement of his height. There may be a tendency for a person to prefer an even 8 or 10 in. height to an odd 9. There is, however, no similar inversion obvious at 4 ft. 8–10 in., although this may be obscured by the effort of men of this height to have themselves recorded as 5 ft.; there is a suspicious bump in the curve just above 5 ft. And the cases at 6 ft. 8–10 in. are too few to show.

The further question arises whether the predilection is simply against the 9 in. or whether all even heights are favored. It is not possible to determine this accurately, since an inversion can not so readily appear in the steeper parts of the curve. If we take the Gaussian distribution (dotted line in the figure) as ideal we see that it is not true that even inches fall above this ideal and odd below. But then it is doubtful whether the Gaussian distribution should be ideal. You can not, at least, prove it from these data, since the probability that an ideal Gaussian distribution would turn out as this observed distribution has is only (by Pearson's chi^2-criterion) about one chance in 10^{88}.

We appear to have, then, a special predilec-

[2] For a complete discussion of the influence of these communal mental habits upon scientific measurements and other quantitative judgments, see J. E. Coover, "Experiments in Psychical Research," 1917, 229–290.

tion in favor of a height of 5 ft. 8 in. or 5 ft. 10 in., or both, which is a function simply of human preference for these heights. The writer would appreciate any information from the readers of SCIENCE which indicates other ways in which this inversion might have been brought about, or which goes to show that persons would tend to regard these two heights as especially desirable.

The instance shows how difficult it is to obtain an "unselected sample" by merely securing large numbers of cases without scientific control of the original observations.

EDWIN G. BORING
CLARK UNIVERSITY

THE PLIGHT OF SCIENTIFIC PERIODICALS

THE world of to-day is in new and trying situations. It is trying its best to meet the conditions imposed upon it by the experiences of the last six years, without relinquishing all of its former ideals.

Its situation is somewhat like that of a family whose house burned last night, but whose clothing and furniture were saved through the energy of the neighbors. Morning has come and reveals the state of confusion. Mother's slippers are in the coffee pot and the lamp shade is full of potatoes. Everything is there, but it will require a long time to bring order out of confusion, at the same time the family must live and maintain domestic peace.

In this country the average man is of the opinion that science had much to do in deciding the military and economic issues from 1914 to 1918. It is not, however, altogether unlike human nature that it should forget its benefactor, though still grateful for the benefactions. Nevertheless, science in this country is to-day in a precarious condition because of the embarrassed financial condition of its professional magazines. It has always been difficult to get scientific work published in this country. The publishers were always politely regretting their inability to publish scientific material, because they had found by experience that it did not pay. The public seemed to have so many other ways in which to spend its money that it didn't want to buy dry books or periodicals.

Science is advanced by research work. That which is discovered is published in technical periodicals or books in the hope that it may advance knowledge and contribute to human welfare. Research work is mainly done by men and women connected with educational or public service institutions. The discoveries which they make are for the benefit of the public rather than the discoverer. Few of their discoveries can be capitalized for personal advantage, and few of the workers have any inclination in that direction. They furnish their original researches to scientific periodicals without receiving reimbursement for them. The main thing the investigator seeks is the opportunity to present his results in an adequate and dignified way to those who may enjoy or use them, and with reasonable promptness.

Consider the situation of the worker in science at the present time. He sends his manuscript to the editor of the *Journal of* ————. In due time he receives some such letter as this:

> We should be glad to publish your manuscript in the *Journal of* ————, but our funds are so restricted that we are obliged to cut down articles as much as possible. In view of the increased cost of printing we want to ask if you would be willing to omit the introduction and first three tables and to combine the other tables into one. We are trying to restrict all articles to ten pages or less of printed matter. The illustrations can not be published unless you will bear the entire cost of plates and paper yourself.
>
> Regretting that such restrictions are necessary, we beg to remain, etc.

The author is obliged to accede to the editor's requests if his paper is to be published and writes accordingly. In about eight months he receives the proof sheets and about a year from the time the manuscript was written the journal containing it is issued. The magazine is thin and pale in appearance. In a few weeks the author gets a package of reprints for which he has to pay at the rate

of 84 cents a page. In addition he is a subscriber to the Journal at $6.50 a year.

In contrast we see that the "popular" magazines flourish as never before and publish beautiful color illustrations galore. We are told that scientific periodicals can not have these things because *they* don't pay." We wonder, Does not science pay to-day as well as in 1917? Will it "pay" to let the scientific world send to Leipzig for its periodicals, rather than to Baltimore?

If scientific publications are to survive and if this country is to support scientific work as it supports other things, there must be some form of endowment for that purpose. Corporations and individuals whose business is even remotely connected with the results of scientific work will find it a good investment in years to come.

The scientists are willing, and do, bear more than their share of the expense of their publications, but outside help is necessary. These periodicals can not expect to pay dividends to the publisher because they are unattractive to advertisers as a class. The technical and scientific periodicals need endowments sufficient to allow them to present adequately the results of research and to enable them to circulate at a subscription price low enough to enable all workers and libraries to buy them.

HOWARD S. REED

CITRUS EXPERIMENT STATION,
RIVERSIDE, CALIFORNIA

ROAD REFLECTIONS

TO THE EDITOR OF SCIENCE: Referring again to the subject of road reflections, Mr. Freemen F. Burr in SCIENCE for September 24 notes having observed reflections occurring at considerable heights above the surface of the road. I have made thousands of careful observations of this phenomenon and have found that the reflecting surface always coincides with the road surface as closely as the eye can determine.

Since the true surface disappears when a reflection takes place there is often an appearance of shifting which careful observation shows to be illusory. Thus a reflecting surface on the top of a hill sometimes seems at a casual glance to be several inches from the road and seems to hide objects beyond. In every such case the hill itself is what cuts off the vision.

I have observed the reflections many times under circumstances that preclude entirely the ascribing of them to warm layers of air. I have seen them on cloudy days, on shaded stretches of road and in one place where a white sign-board furnishes a convenient background a very striking reflection may be seen long after sunset.

To be sure they are much more in evidence on bright days than on dull days, but since they appear even more brilliantly on a very cold bright winter day with snow on the ground than on a warm summer day the conclusion to be drawn is that the contrast of bright colors throws the reflections into more prominent relief on sunny days.

The same phenomenon may be observed by holding any smooth normally non-reflecting surface, such as that of tarnished metal or of a smooth whetstone, at a small angle to the line of vision. Objects beyond appear brilliantly reflected as if in a mirror.

It may be that in some instances a thin air layer immediately adjacent the surface aids by bending some incident rays so that they strike within the critical reflection angle. But the air layer certainly is never primarily responsible for reflections of this kind.

H. H. PLATT

SCARSDALE, N. Y.

THE INFLUENCE OF FRESH FOOD IN LACTATION

THE suggestion of Hart, Steenbock and Hoppert, in the October 1 number of this journal, that a vitamine in fresh grass favorably influences calcium metabolism is a step in a direction in which, I am convinced, important progress is to be made.

Through extensive investigations on the mineral metabolism of farm animals I had reached a hypothesis identical with the provisional conclusion of Hart and associates,

and had planned work to reveal the facts. Fortunately the department of dairy husbandry of this institution, under the leadership of Professor C. C. Hayden, has had in progress for many years an investigation with dairy cows contributing evidence directly on this subject; and during the past summer the writer has had under observation this same group of cows on which he was conducting mineral feeding experiments.

Mr. Hayden permits me to make the following observations on these cows. Since 1911 a group of Holstein-Friesian cows has been maintained on dry feeds and silage alone. No green feed has been allowed. Several of the animals now in this group were born in the same and have grown to full maturity without having a bite of green feed. Extensive evidence is at hand, therefore, as to the importance of the suggestion of Hart and associates, as bearing on lactation in cattle.

This group of cows has grown to normal weights, and has produced and reared calves without marked or certain irregularity or abnormality. The milk production has been fair only, it being obvious that with normal treatment these cows would have given more milk. They do not have normally keen appetites and some are easily forced off feed. They will not eat enough feed to support maximum milk production. They fall away during lactation a little more than is customary, but pick up again after going dry. These cows have been in noticeably less thrifty condition, as indicated by flesh and coats, than the balance of the herd, which goes to pasture, and it has been apparent that they crave something which they do not find in the ration.

During the past summer the writer has conducted palatability tests on these cows, with various mineral supplements. They have manifested a keen desire for mineral feeds, having eaten, in several short periods, from approximately three fourths of a pound to more than one and a fourth pounds of mineral supplement per head per day. These supplements have consisted of various calcium phosphate and carbonate preparations, fed alone or mixed with common salt. It has been shown that the desire of the cows for these feeds has not been wholly or predominantly due to the liking for salt. The avidity with which these cows eat steamed bone, considered in connection with our finding that negative calcium balances normally prevail during lactation in cows on dry feed, and the conclusion of Hart and associates that fresh grass favorably affects calcium retention, suggests that these dry-fed cows are in a state of mineral depletion, especially while giving milk.

It appears, therefore, that the suggestion of Hart and associates is a matter of practical importance as relating to milk production, but that rations of dry feeds and silage, though probably deficient in some constituent, are not entirely lacking in any essential.

The most important work indicating the dependence of calcium metabolism, especially of the teeth, upon vitamines, which has come to the writer's attention is an extensive and unusually successful investigation with guinea pigs (as yet unpublished) by Dr. Percy R. Howe, of Harvard University.

E. B. Forbes

Department of Nutrition,
Ohio Agricultural Experiment Station

SWARMING OF ANOPHELES

To the Editor of Science: It may seem a little late to publish this note, but, on account of a long absence from the country, I have only just seen reference, in the *Review of Applied Entomology* for May, 1920, to Mr. C. S. Banks's article entitled "The Swarming of Anopheline Mosquitoes" published in the *Philippine Journal of Science* for September, 1919. Mr. Banks is quoted as stating in the article that, although the swarming of mosquitoes has been constantly reported, "no mention seems previously to have been made of this habit in the case of Anophelines." As a matter of fact the late Mr. Frederick Knab published in *Psyche* for February, 1907, a rather extended note on the swarming of *Anopheles maculipennis* Say. This note is reprinted in full in the Monograph of the

Mosquitoes of North and Central America and the West Indies, published by the Carnegie Institution of Washington, and in that connection observations of a somewhat similar character by several other authors are mentioned (see pages 126–129, Vol. I. of the Monograph).

L. O. HOWARD

THE WORKS OF AMEGHINO

THE Minister of Public Works of the Province of Buenos Aires, Argentine Republic, is financing the publication of a complete edition of the scientific writings and correspondence of Florentino Ameghino (1854–1911) the distinguished South American paleontologist. The editorial work has been undertaken by Alfredo J. Torcelli, and Volumes I. and II. have been issued; though printed in 1913–14 they have just been received. Volume III. will be devoted to "The Antiquity of Man in La Plata," originally issued in two volumes in 1880–81.

The publication of such a work, stupendous as it is, will prove of inestimable value to those workers who entered the field after Ameghino had published his first papers, copies of which are rarely found in an ordinary scientific library. His writings number 179 essays and books written in Spanish, French and English; some of them large volumes. Ameghino was a voluminous writer and he seldom published short papers. While the number of papers is not large compared to some European writers; Hermann Schaaffhausen for instance, wrote 314 contributions along the lines of anthropology; yet in content they compare favorably with the productions of any one scientific writer of modern times.

The first volume is entitled "Vida y Obras del Sabio," an octavo of 391 pages, printed on a poor quality of paper, and containing a complete account of the life and activities of this noted South American scholar. There is likewise appended a description of the elaborate funeral ceremonies with which his native city nourned the loss of this eminent man.

The second volume with the title: "Primeros Trabajos Cientificos," is much larger, comprising 770 pages. One regrets the poor quality of the drawings; but it must be remembered that Ameghino's drawings, of which there were thousands, were made by his own hand, untrained to do such work, and under unfavorable conditions, working in the back room of his stationery shop in La Plata. The drawings originally poor and not well produced at first, are not all we would like, but are still of great value as an aid in interpreting Ameghino's ideas.

The third volume will comprise the XXIVth memoir, and since there are 154 memoirs to follow one can, with a little simple mathematics, compute the approximate size of the series. An interesting sidelight to Ameghino's restless mental activity is given in Memoir XXIII., with the title "Taquigrafia Ameghino," which appears to be an attempt on his part to reduce the Spanish language to shorthand; the characters having a marked resemblance to those used in some American shorthand systems. It is to be hoped that his shorthand system was successful. But of its use in the commercial world of Spanish-speaking peoples, I know nothing. How many living paleontologists have devised a system of such far-reaching importance to another world than their own?

Florentino Ameghino was a wonderful man, and I am sure we wish Alfredo J. Torcelli all the success in the world in his "honorable autant que difficile travail."

ROY L. MOODIE

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CHICAGO

SPECIAL ARTICLES

THE FREE-MARTIN AND ITS RECIPROCAL: OPOSSUM, MAN, DOG

IN 1917 the writer purchased a large fat opossum, presumably a male, but actually a sex-intergrade possessing the following characters: externally, normal penis, empty scrotum, small malformed pouch, head rather like that of a female; internally, reproductive organs distinctly of the female type, infantile

in development, consisting of vaginal canals, uteri, Fallopian tubes, and small round bodies in the situation of the ovaries. These bodies were sectioned and found to contain within a thin albuginea a mass of closely packed tubules of uniform size, devoid of all indication of sex cells, male or female.

This specimen is presented in further evidence of the hormone theory of sex differentiation, in the light of which it is interpreted. The explanation here offered was suggested by Lillie's beautiful demonstration of the cause of the free-martin in cattle,[1] wherein he showed beyond all doubt that the free-martin is a female sterilized by sex hormones from the male co-twin in all cases, and only in such cases, in which the fetal circulations anastomose and mutual blood transfusion occurs. One type of pseudo-hermaphroditism is thus adequately and simply explained and Steinbach's[2] assumption of an embryonic gonad containing both male and female interstitial cells as the origin of the antagonistic sex hormones in fetal life becomes superfluous.

The opossum briefly described above is interpreted as a reciprocal free-martin, a term I wish to employ for a sex-intergrade which is zygotically *male* but which in its ontogeny develops *female* characters. I venture to suggest that it has arisen by "inhibition and stimulation of normal embryonic rudiments"[3] through the influence of female hormones from a female co-twin. It is believed, therefore, that the case constitutes an answer to the following significant statement by Lillie:

> On the male side there is complete absence of information as to the effects of early embryonic castration and the possible effect of the presence of female hormones in the absence of male hormones.[4]

The writer acknowledges, of course, that he is arguing from analogy; and he would himself interpose the further objection that such sex-intergrades are very rare, at least in adults of the opossum. And yet, when one opens a pregnant opossum uterus and observes that it is crowded to capacity with vesicles under great pressure and that the chorions are in mutual contact by large surfaces, he must wonder why anastomoses of the chorionic circulations do not more frequently occur. In this extreme crowding the marsupials are unique. It has occurred to the writer as highly probable that the shell membrane of marsupial eggs, far from being a useless vestige of Sauropsidan ancestry, serves to separate the vesicles until it is safe for them to come into immediate contact, if indeed they ever do. This point will be the subject of further investigation, as also the mechanism by which the male embryo is protected against the hormones of its mother and of its sister embryos. There being no placenta in the opossum, this organ is ruled out in this case.[5]

There is one condition, however, under which in the opossum fusion of the chorions is likely to occur, namely in twin eggs where two ova are included in the same egg envelopes. I have several such eggs in different stages of development.[6] A comparable case has been described for the rat where fusion took place between the trophoblasts of two eggs that became included in the same decidua.[7] Professor H. M. Evans, of the University of California, told the writer that among several hundred dog embryos of his collection he found one pair of very young twins within the same uterine swelling. The placentæ were fused (v.i.). These points raise interesting questions as to the mechanism by which the ova are usually distributed singly in oviduct and uterus.

The sex-intergrade of the opossum here pre-

[1] F. R. Lillie, *Jour. Exp. Zool.*, 23, 1917, 391–452.

[2] E. Steinach, *Pflüger's Archiv*, 144, 1912, 71–108 (see especially page 86).

[3] Lillie, *l. c.*, page 419.

[4] Lillie, *l. c.*, page 415.

[5] *Cf.* Lillie, *l. c.*, footnote, page 415.

[6] See Carl G. Hartman, *Jour. of Morph.*, 32, 1919. One twin egg may be seen in each of the batches shown in Fig. 4, Pl. 9 and Fig. 2, Pl. 1.

[7] V. Widakowich, *Zentralbl. f. Physiol.*, 24, 1910, 305.

sented furthermore corroborates in a striking manner the findings of Lillie (l.c.) and Chapin[8] that the hormone influence makes itself felt in the earliest stages of sex differentiation. That this must be so is proved by the fact that this animal is born ten days after fertilization and five days after the primitive streak stage.

Again, since in the specimen the cortex of the gonad is seen to be entirely absent we have here a confirmation of Lillie's statement that the gonad of the zygotic male, not possessing the homolog of the cords of Pflüger, is capable of less transformation than the embryonic ovary.[9] The absence of male sex cells in the specimen and the presence of healthy tubules (probably Sertoli cells only) is precisely in keeping with the theory of the influence of the female hormone in fetal life.

The assumption of certain embryologists that the embryo in the undifferentiated stage is a true hermaphrodite, is, therefore, no longer tenable.

More crucial evidence in favor of the view expressed above is, however, furnished by the following human case described by Eschricht.[10] It concerns a sex-intergrade which in all essentials was an exact counterpart of the opossum described above: externally, penis and empty scrotum; internally, uterus, Fallopian tubes with fimbriæ, and atypical "ovaries." It was a reciprocal free-martin, who, because of other malformation, died a few minutes after birth. The significant facts, however, in this human case are: (1) that *the child was born co-twin to a normal female* who lived and (2) that *the placentæ* were fused ("sehr genau verbunden"). Better proof could hardly be desired. I refrain from mentioning other human cases that must be interpreted as reciprocal free-martins, as, *e. g.*, those cited by Simpson.[11]

From the same article by Simpson it seems clear that the true free-martin also occurs in man. Such cases the author classifies with the free-martin of cattle, showing that he correctly interpreted them. This occurrence of both types in the same form (man) need constitute no great obstacle to the hormone theory, for it is quite conceivable that sometimes the male, sometimes the female co-twin gets the start in development, since the handicap need be very slight to prove ruinous to the laggard.

That the free-martin also occurs in rare instances in the dog, a multiparous animal, seems probable, since the "hermaphroditic dog" described by Home[12] in an apparently forgotten article is almost certainly a free-martin of the cattle type. In this connection the case of fused placentæ of the dog embryos found by Dr. Evans and cited above is of more than passing interest.

Free-martins, reciprocal free-martins and intermediate conditions may, therefore, be expected to occur in all mammals. The principle of hormone influence in fetal life, first demonstrated by Lillie,[13] constitutes the most important contribution to the subject as yet made. Twins and double monsters will have to be reclassified in the light of the theory[14] and such monographs as those of Sauerbeck and of Hübner[15] will have to be largely rewritten.

A more complete paper will be published later.

CARL G. HARTMAN
THE UNIVERSITY OF TEXAS

[8] Catherine L. Chapin, *Jour. Exp. Zool.*, 23, 1917, 453–482.

[9] Lillie, *l. c.*, page 419.

[10] Eschricht, *Müller's Archiv*, 1836, 139–144.

[11] Sir J. Y. Simpson, article "Hermaphroditism" in Todd's Cyclopædia of Anat. and Physiol., 1836–39.

[12] Everard Home, *Phil. Trans. Roy. Soc.*, 1799, 162.

[13] See F. R. Lillie, SCIENCE, N. S., 50, 1919, 183–184.

[14] *Cf.* E. Steinach, *Archiv f. Entwicklungsmech.*, 42, 1916, 307–332.

[15] *Ergebn. d. allg. Path.*, 15, 1911.

THE NEBRASKA ACADEMY OF SCIENCES

THE Nebraska Academy of Sciences held its thirtieth annual meeting at Doane College, Crete, Nebraska, on April 30 and May 1. The plan is to meet in Lincoln every other

year and at some other point in the state on alternate years. Usually the attendance is larger when the meetings are held in Lincoln as so many members are connected with the university and the Lincoln colleges. When held outside of Lincoln, there are usually interesting field trips and the smaller number makes possible a closer personal contact.

On Friday evening, the members were served the evening meal by the domestic science class. For the other meals, tables were set apart for the academy members at the college dining hall.

Following is the program:

Friday, 1:30 P.M.

Notes on the anatomy of Okapia johnsoni: H. V. Von W. Schulte.

The two classes of sperm in Rotifers: D. D. Whitney.

The use of the aeroplane in studying vegetation: P. B. Sears.

Equisetum gametophytes in Nebraska; A new species of Obedokonium: Edna R. Walker.

Root systems of cereal crops in the grassland formation: J. E. Weaver.

Dissemination of fungi with special reference to that of Sphærobolus and related forms: Lena B. Walker.

Pioneer tales from southeastern Nebraska. A sketch of Nebraska's early newspapers: Unice Haskins.

More western traditional songs: Louise Pound and Eleanor Burkett.

Racial elements in Nebraska population: A. E. Sheldon.

A scientific study of Czechoslovakia: Rose B. Clark.

The psychological clinic in practice: G. W. A. Luckey.

6 P.M.

Banquet and social hour.

8 P.M.

President's annual address, climate and evolution (illustrated.)

Saturday, 9 A.M.

Business Session.

10 A.M.

Some lessons in fuel conservation: J. C. Jensen.

Some investigations in the transmission of heat through boiler tubes: Jiles W. Haney.

Development of the telephone: V. L. Hollester.

Light and gravitation: H. H. Marvin.

At the business meeting final action was taken to affiliate with the A. A. A. S. and plans made to better organize the science work of the state. The following officers were elected:

OFFICERS

Dr. Elda R. Walker, *President.*

Professor A. J. Mercer, *Vice-President.*

Professor W. F. Hoyt, *Secretary.*

Dr. G. W. A. Luckey, *Treasurer.*

Lincoln Academy of Medicine.

Dr. E. G. Zimmerer, *Secretary.*

The executive committee held a meeting in Lincoln on August 28 and planned a campaign for membership. Members of the A. A. A. S. and of the N. A. S. will be invited to come in under the affiliated membership plan if they have not already done so. The final arrangements were made for the affiliation of the Lincoln Academy of Medicine with the N. A. S. The president announced the following appointments for sectional vice presidents:

SECTIONAL VICE-PRESIDENTS

Biological and Medical Science, Dr. R. A. Lyman.

Mathematical and Physical Science, Professor J. C. Jensen.

Ethnology and Folklore, Dr. Louise Pound.

Engineering, Professor George R. Chatburn.

Earth Science, Professor N. A. Bengtson.

C. O. Carlson

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